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NOTES AND LITERATURE

PATTEN ON THE ORIGIN OF VERTEBRATES, AND THE GENERAL QUESTION OF THE VALUE OF SPECULATIONS ON THE PHYLOGENY OF ORGANIC BEINGS[1]

FROM the standpoint of range of topics covered, amount of work performed, and time devoted to its execution, this work by Patten may without exaggeration be spoken of as monumental. Many of the facts set forth are original observations by the author and his students, and of those not original a large proportion have seemingly been personally studied by him. Furthermore, nearly all the large number of figures are either original or bear the stamp, by way of modification of borrowed figures, of Patten's well-known skill as an illustrator.

A list is appended comprising 26 titles of papers and addresses by the author or the author in collaboration with his students; but unfortunately references to the works of other investigators drawn upon are rather few, often somewhat indefinite, and not well set out in the text. In a book so abounding as is this one in argumentation, many of the main contentions of which are open to debate, sources due to authority ought to be given exactly and without stint.

Frequent as is the occasion in scientific books to estimate worth from the two viewpoints of facts presented and theories defended, rarely is the importance of keeping the two distinct so great as in this case. Many of the chapters, notably V to XII and XVI to XX, are veritable magazines of recorded observation to which workers in the field will, it would seem, find it profitable to turn for years to come. This remark applies particularly to the sections dealing with the central nervous system of *Limulus;* with the cutaneous, olfactory, and optical organs of "Cerachnids" and Anthropods; with the dermal skeleton of *Limulus;* with the endoskeleton of Arachnids; with the nerve supply to the heart of *Limulus;* and with the general structure of the Ostracoderms.

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[1] "The Evolution of the Vertebrates and their Kin," by Wm. Patton, 486 pp. and 309 figures, P. Blakiston's Son & Co., 1912.

The experiments on functions of the brain recorded in Chapter XI should be a valuable contribution to the interesting problem of metamerism as expressed through the activities of the central nervous mechanism.

Two defects in the descriptive matter are likely to interfere with as extensive a utilization of the book by biologists as it merits. The first to be mentioned is a want of directness and definiteness in many of the descriptions that renders their comprehension extremely difficult, in some cases almost impossible. This is due partly to the way references are made to the illustrations. Not infrequently a text description of a structure is given, not very fully, in the course of which one or several figures are referred to but without specifying the letterings for the particular parts described. The reader, being in doubt, may turn to the "Explanation of lettering" at the end of the volume, only to find that the illustrations in question either have no letterings for the particular parts, or if sufficient patience in digging is exercised, to find that the part is labeled with a different name from that used in the description. The account of the "middle cord, the lemmatochord and the notochord" (Chap. XVIII) is an example of the difficulty here indicated. Although I have spent much time on this chapter, I have not been able to get a clear understanding of what is dealt with. How many distinct structures are in hand? "The beginning of the notochord may be recognized in practically all segmented invertebrates, as the so-called middle cord, or median nerve, and in its derivative, the lemmatochord," p. 324. This statement is general, *i. e.*, is not made as applying to any particular animal. It seems definite to the effect that "median nerve" and "middle cord" are synonymous, and that the structure indicated gives rise to the lemmatochord. But Fig. 224*A*, p. 327, representing the "nerve cords and lemmatochord of *Cecropia*," presents to us the "late pupal stage showing the fully formed lemmatochord, derived from the condensed *sheaths* of the median and *lateral cords;* also remnants of the median nerve." (Italics by the reviewer.) In *Limulus* the "middle cord" is said, p. 334, to be "arranged in two main lateral cords," and Fig. 55, *l.l.ch.*, p. 67, is referred to as illustrating this statement. Turning to this figure and the explanation of letterings, we find that *l.l.ch.* stands for "lateral bands of the lemmatochord." This same figure shows a "median portion of the lemmatochord" distinctly and widely separated from the lateral portions. The inference is that these lateral and median structures unite somewhere; but no direct statement to this effect is given, at least in the section on the middle cord of *Limulus*.

Again, Fig. 225, p. 328, presents five cross sections of the nerve cord of an adult scorpion. Section 5 is said to show the "merochord." Neither in the description of this figure nor in the text do we find a direct letter reference to the merochord. A structure labeled *m* appears in the figure, but on turning to the explanation of lettering *"m"* we find may stand for "mouth" or "muscle." But it is unfair to criticize illustrations and their letterings and labels alone. They must be taken in connection with the text. By reading a subsection headed "The Bothroidal Cord or Lemmatochord" we find that the merochord in section 5 of Fig. 225 is marked *l.ch.*, which stands for lemmatochord. A sufficiently careful reading of the text clears up the merely expositive difficulties contained in the figure: The merochord is the lemmatochord of the "posterior thoracic neuromeres," p. 328. This interpretation is compelled when Fig. 5 is taken in connection with the text statement indicated. But then the difficulty becomes substantial and not merely expositive, for on page 328 we read: "In the scorpion, the median nerve itself is hardly recognizable. . . . The neurilemmas of the median and lateral cords form the bothroidal cord of the abdomen and the merochord of the posterior thoracic neuromeres." But we have seen above that according to section 5 of Fig. 225 the merochord is a particular part of the lemmatochord. Hence this part at least of the lemmatochord is formed from the neurolemmas of the median and lateral cords. Under the topic "Development of the Lemmatochord" we read "The lemmatochord arises, in part, as an axial cord of cells extending forward from the primitive streak"; and nothing under this heading or elsewhere so far as I have been able to find, harmonizes this statement with the indirect assertion above pointed out that the lemmatochord is formed from the neurilemmas of the median and lateral cords. The nearest approach to such harmonization is the statement, p. 330, that at the time of hatching, the lemmatochord at certain places "remains permanently attached to the neurilemma of the middle cord." As one of the many students who have expended considerable time and "gray matter" on the problem of the forerunner of the vertebrate notochord the reviewer would heartily welcome a demonstration that the organ "may be recognized in practically all segmented invertebrates"; but until a clearer, more convincing *description* is furnished us of some structure in "practically all segmented invertebrates" with which the vertebrate notochord is to be compared, the question of homology in the strict sense would not even be raised were the best interests of comparative anatomy duly considered.

The other defect in the presentation of matters-of-fact which a well-wisher for the book may justly fear will tend to prevent as wide use of it as it deserves, is the circumstance that several modes of statement occurring over and over again are bound to give even the fairest-minded reader the impression that many of the facts were prejudged; that is, were collected and recorded not primarily on their merits, but in behalf of a theory. The nomenclature employed in several important connections is likely to have this effect. The use of the substantive *cephalon* with various prefixes in purely descriptive matter dealing with the thoracic region of arthropods is an example.

In the chapter "Minute Structure of the Brain and Cord of Arachnids" we read (p. 80):

> The more posterior thoracic commissures, and those in the hindbrain, are shorter, and the neural and hæmal fascicles are widely separated, leaving a space between them, *which represents the beginning of the fourth ventricle* (italics by the reviewer).

Such dogmatic and uncalled-for statements inserted into purely descriptive matter are very unfortunate, for they can but militate against the factual value of the work in the mind of every candid reader. The feeling of uneasiness engendered in the reader by these gratuitous dogmatizings, as to the extent to which facts dealt with have been unconsciously colored by theory, is not allayed by the author's avowed attitude toward the facts of organic structure. On page 469 we find this:

> Comparative morphology has no value except in so far as it points out the historic sequence of organic forms and functions, and reveals to us the trend of evolution and the causes that direct and control it.

This statement I insist is not true. It may be partly true, but in the unqualified form given it by the author is not only untrue, but is provokingly and harmfully untrue. If for Dr. Patten comparative morphology has no value except in the ways indicated, well and good. I neither question nor quarrel with the assertion. For me, however, comparative morphology has great value in numerous other ways; and there is much evidence, both historical and contemporaneous, that it has other values for many biologists. It is, I submit, a real even though unintentional harm, not only to individuals, but to biological science, for an able morphologist to make an assertion which carries the clear implication that the interest in and the valuations placed upon comparative morphology by the men who devoted themselves to it long before anybody knew there was such a thing as a ''trend of evolution,'' were an illusory or spurious interest and valuation. And I would express my firm conviction that biologists of this present childhood period of the evolution theory, as the era from Darwin to the present day may well be called, must come to see that great as is the value of morphology as a record of evolution, this is still *only one* of its values; and further, that until such perception is attained, just estimation of the facts of morphology *as a record of evolution* will be impossible. The interest of the future morphologist in his raw material ought to be, according to my understanding, that of the pre-evolutionary morphologist *plus* that growing out of the later discovery that the facts of structure mark the ''historic sequence of organic forms and functions.'' It is just because many, indeed all of the facts dealt with in this work have values for me over and above those attaching to them as a record of evolution that I regret that they could not have been presented in a fashion less calculated to raise doubts in so many instances as to whether they would appear exactly as they do but for the circumstance of having been interpreted by the author in the light of his particular theory of historic sequence.

This consideration will, I trust, give real weight to my words when I say that the criticisms I am passing on Patten's mode of presenting facts is an apology for a work of truly great factual worth, and not at all an attempt to discredit it. Nor would I have any one understand me to be an advocate of ''mere facts'' of morphology; facts, that is, without any reference to their wider bearings. My point is that all facts of morphology, as of all other departments of biology, have so many ''wider bearings'' that to write down as without value all except some one set of these bearings, even so important a set as that of evolution, is to narrow the horizon of biological science. Against tendencies of this sort, wherever occurring, and unfortunately they occur in many sections of the vast realm, I am ready at all times to do battle.

So far this review and commentary has been made entirely from the standpoint of observed and observable facts dealt with in the volume. Now the point of view must be shifted to the theoretical side.

Patten's central thesis, as is well known, is that vertebrates have descended from arachnids. The "arachnid theory of the origin of vertebrates," or, for short, the "arachnid theory," is the phraseology used by the author. The theory was first definitely set forth in 1889, the title of the original publication being "On the Origin of Vertebrates from Arachnids." A somewhat abbreviated quotation of the author's outline of the theory will be justifiable. He writes:

> This theory has formed the basis of all my subsequent work, and as far as it went, is practically the same as the one presented here. In that paper it was maintained that the vertebrates are descended from the arachnid division of the arthropods, in which were included the typical arachnids, the trilobites, and merostomes. The ostracoderms were regarded as a separate class, uniting the arachnids with the true vertebrates. *Limulus* and the scorpion were the types most carefully studied, because they were the nearest and most available living representatives of the now extinct merostomes, or giant sea scorpions, that were regarded as the arachnids standing nearest to the ostracoderms.
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> Other evidence and conclusions were as follows: (1) In the arachnids a forebrain vesicle is formed by the same process of marginal overgrowth as in the vertebrates. . . . (2) The kidney-shaped compound eye of arachnids has been transferred to the walls of the cerebral vesicle in vertebrates, giving rise to the retina, which still shows traces of ommatidia in the arrangement of the rod-and-cone cells. . . . (3) The arachnids have a cartilaginous endocranium similar in shape and location to the primordial cranium of vertebrates. (4) They have an axial, subneural rod comparable with the notochord. (5) In arachnids the brain contains approximately the same number of neuromeres as in vertebrates. . . . (6) The segmental sense organs (median and lateral eyes, olfactory and auditory organs) are comparable with those in vertebrates. The coxal sense organs are associated with special sensory nerves and ganglia, comparable with the cranial dorsal-root nerves and ganglia (suprabranchial sense organs) of verterates. (7) The basal arches of the appendages are comparable with the oral and branchial visceral arches in vertebrates. (8) The tendency toward concentration of neuromeres has narrowed the passage way for the stomodeum and modified the mode of life in the arachnids. This ultimately led to its

> permanent closure, the infundibulum and adjacent nerve tissues in vertebrates representing the remnants of the old stomodeum with its nerves and ganglia. . . .[1] (12) The process of gastrulation in vertebrates and arachnids is confined to the procephalic lobes, in the place where at a later period the primitive stomodeum appears. The so-called "gastrulation" of vertebrates and arachnids is an entirely different and independent process, that is, the process of adding by apical or teloblastic growth a segmented, bilaterally symmetrical body to a primitive radially symmetrical head. (13) The arachnids resemble the vertebrates in more general ways, as in the minute structure of cartilage, muscle, nerves, digestive, and sexual organs. (Pp. xvii and xviii of the Introduction.)

None of the statements in this list, either those quoted or those not quoted, bring out clearly one of the most striking features of the theory, namely, the supposition that the ventral surface of the arachnid became the dorsal surface of the vertebrate. This is well shown by several series of figures, as, for instance, that on page eight, of imaginary arthropods and vertebrates with creatures intermediate between them.

During the twenty years and more that the theory has been before zoologists it seems to have won very few adherents; indeed, to have had little influence on biological thinking of any sort. Nor does it seem probable that this final marshaling of the evidence will accomplish much more as regards the main contention. So far are we still from certainty as to exactly how and when the back-boned animals originated that even the probable evidence toward such knowledge is not great. Indeed, if one will consider fully the nature and difficulties of the problem he will see that the chance of ever reaching certainty is almost nil.

What would constitute a demonstration of the parenthood of vertebrates? Obviously the most indubitable proof, that of direct observation, is out of the question. The "supreme canon of historical evidence that only the statement of contemporaries can be admitted," must, in the nature of the case, be completely ignored here. The best chance of reaching a demonstration is in finding a series of fossil animals intermediate between some unmistakably primitive vertebrate and the assumed ancestor, containing no gaps great enough to raise serious doubt in the mind of any competent authority as to the genetic relationship

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[2] Number nine in this list is absent in the text. Numbers 10 and 11 are purposely omitted in the quotation.

of the two forms separated by the gap. The greatness of our lack of a series connecting any vertebrate with any invertebrate whatever, must impress one more and more the more he knows and thinks about the problem. The only other conceivable mode of demonstration would be to induce, experimentally, the transformation of some living invertebrate into an undoubted vertebrate. The possibility of accomplishing such a fact is so slight that no biologist is likely to try it.

This brings us to a point where the transcendent importance comes to view, of the logic of the interpretation of evolution, understanding by evolution the transformation of one kind, or species, of organism into another kind, or species. Except in the relatively simple and rare cases of proof through direct observation, experimental or other, the fact that all evidence rests back absolutely on resemblance—on similarity in form and composition of the parts of organisms—is of the utmost importance. No evolutionist hesitates in either word or intention to accept this tenet; yet when it comes to the actual ascertainment of likenesses and differences, and to speculating on their phylogenetic significance, the danger of shifting from the inductive to the deductive mode of reasoning, and of going down before the fallacy known in the logic books as *petitio principii,* or *surreptitious assumption,* is so imminent and subtle that very few of us, even those most wide awake for pitfalls, avoid it wholly. Almost if not quite all the numberless hypothetical ancestral organisms that have been summoned to the aid of speculation on descent during the last half century and more, are, I am convinced, victims of this evil, some in greater, some in less degree. The fallacious reasoning usually runs something like this: Assuming such-or-such an animal once existed, it is easy to see how a particular organ or part of the animal actually before us may have arisen from that ancestry. Says Patten:

> We have merely to strip off the superficial disguise of our hypothetical arachnid ancestors and see whether either their underlying structure, their mode of growth, . . . does or does not harmonize with the assumption that they are the ancestors of the vertebrates (p. 3).

Before this "mere" stripping off begins, would it not be well to consider "our hypothetical arachnid ancestors" rather closely? *If* such an animal actually once existed, let it be granted for the moment that it might have undergone such a transformation as is conjectured. Surely the first point to be pressed is as to the evidence that the supposed creature did exist. And what is the evidence? Why, the observed facts of arachnid structure, exactly these and no others, else the ancestor would not be held as *hypothetical*. No hypothesis can, of itself, add any new facts. The hypothetical ancestor has, consequently, done nothing for the case except to disguise the obvious difficulty there is in seeing how an *actual* arachnid can be transformed into an *actual* vertebrate.

The only really safe rule for using hypotheses in biology is that no hypothesis shall be made except to help toward answering a question by formulating a clear *provisional* answer to *that question*. The making of hypotheses and using them *before they are themselves proved* for the solution of other problems than those to which they immediately pertain, is perilous business. I may without danger, even with profit, construct an imaginary enteropneust to aid my efforts to answer the question of how the enteropneustic branchial apparatus or the organ in this group called a notochord, arose phylogenetically. But before this imaginary creature can do me real service I must establish at least a strong probability that such an imaginary animal once actually existed. Nor must I fail to notice that such probability can be established only by bringing new evidence into the case. The facts on which I based the hypothetical animal can not be used over again to prove the reality of that animal. But if I must be thus cautious in resorting to an hypothetical enteropneust for the interpretation of actual enteropneusts, how much more need would there be for caution should I venture to invoke an hypothetical enteropneust for interpreting actual vertebrates!

Reflections of this nature opened my eyes several years ago when I was struggling with the enteropneust hypothesis of vertebrate descent, to the very slight chance there is of ever solving the problem of vertebral origin.

Do I then regard all hypotheses on the problem as equal in value because all alike are equally futile? By no means. I consider each one of them to be of real worth, particularly those that have been worked out as have Patten's arachnid hypothesis, Gaskell's crustacean hypothesis, the annelid hypothesis and the enteropneust hypothesis. They are of worth because each shows quite clearly certain *possible* developmental courses that may have been followed in the origin and progress of the great vertebral stock. To be explicit, it seems to me that Patten has shown that in certain structural details there is a similarity between the exoskeletons of *Limulus* and some of the Ostrachoderms which makes quite possible, not somewhat probable, genetic relationship between these animals. Similarly the remarkable likeness between the gills of the enteropneust and amphioxus, if considered by itself, would make genetic kinship between these animals highly probable; however, when considered along with the whole organization and mode of life of each group, the probability is much reduced, to such an extent, indeed, as to become hardly more than a strong possibility.

If we take due cognizance of the extent to which our faith in the general theory of organic evolution rests on similarities of form and function among individual organisms which come under our observation, we are in position to feel the weight of the purely inductive evidence furnished by the many resemblances between vertebrates and invertebrates brought to light by the investigators in support of the various hypotheses of vertebrate ancestry, that not only is the theory of evolution true as applied to the back-boned animals, but that the ancestors of these animals must have been in many respects like certain invertebrated animals with which we are familiar. The inductive proof of the truth of organic evolution, even of the general course of evolution within large provinces of the living world, approaches much closer to certainty when taken *en masse* than does, of necessity, that pertaining to any single instance or small group of instances.

So the moral drawn from examining the theoretical side of the volume before us has this in common with that drawn from examining the factual side: It has real worth, but that worth would stand forth much more sharply, and probably would be received by biologists with far greater sympathy, had it been always and clearly distinguished in presentation from pure matters of fact. And the worth would have been greater still had the main and numerous subsidiary and corollary hypotheses been frankly treated as far from demonstrated or even demonstrable, but as varying in degree of reasonableness from very *possibly* true to rather *probably* true.

Wm. E. Ritter

La Jolla, Calif.,
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